Philosophia Scientiæ
Travaux d'histoire et de philosophie des sciences

15-3 | 2011
L'espace et le temps

The Constitutive and the Conventional in Poincaré's Conventionalism

Steven Bland

Electronic version
URL: http://journals.openedition.org/philosophiascientiae/680
DOI: 10.4000/philosophiascientiae.680
ISSN: 1775-4283

Publisher
Éditions Kimé

Printed version
Date of publication: 1 October 2011
Number of pages: 47-66
ISBN: 978-2-84174-569-2
ISSN: 1281-2463

Electronic reference
Steven Bland, « The Constitutive and the Conventional in Poincaré's Conventionalism », *Philosophia Scientiæ* [Online], 15-3 | 2011, Online since 01 October 2014, connection on 30 April 2019. URL : http://journals.openedition.org/philosophiascientiae/680 ; DOI : 10.4000/philosophiascientiae.680

The Constitutive and the Conventional in Poincaré's Conventionalism

Steven Bland
Huron University College (Canada)

Résumé : Parmi les arguments contre la possibilité d'une distinction de principe entre le factuel et le conventionnel, l'un des plus suivis affirme que, parce qu'on ne peut évaluer ses convictions que collectivement, on peut soutenir n'importe quel énoncé face à n'importe quelle expérience. Cet article propose d'établir que cet argument ne peut pas ébranler le conventionnalisme de Poincaré, étant donné que sa doctrine ne se réduit pas à l'affirmation qu'il y a des principes immunisés contre la révision. On soutiendra que le point le plus important de la compréhension de la structure de la connaissance théorique par Poincaré réside dans le fait que certains principes ne peuvent pas être testés expérimentalement parce que ce sont les principes mêmes qui permettent l'application des termes théoriques à l'expérience.

Abstract: One of the most influential arguments against the possibility of drawing a principled fact-convention distinction consists in the insight that because our beliefs are necessarily evaluated together, *any* statement can be retained or given up in the face of experience. The purpose of this paper is to establish that this argument does not undermine Poincaré's conventionalism in virtue of the fact that this doctrine does not simply amount to the claim that there are principles that are immune to revision. It will be argued that Poincaré's most important insight into the structure of theoretical knowledge is that there exist principles that cannot be empirically tested because they make possible the application of theoretical terms to experience.

The most influential argument against the possibility of *a priori* principles to come out of the twentieth century stems from the insight that "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" [Quine 1961, 41]. This is the doctrine of epistemological holism and it has been argued that if it is true, then *any* statement can be made immune from revision, and at the same time, *no* statement is beyond getting revised, should we find it pragmatically advantageous to do so. Consequently, the argument goes, the distinction between empirical and *a priori* claims, or factual and conventional statements, will be at best arbitrary (given the first insight), and at worst non-existent (given the second). This argument has been used by Quine to attack the dogma of analyticity and, more recently, by Devitt to undermine the possibility of *a priori* knowledge (see [Quine 1961], [Devitt 1998], and [Devitt 2005]). It was used earlier by Duhem to attack Poincaré's doctrine of conventionalism. The purpose of this paper is to show that the argument from epistemological holism fails to undermine Poincaré's conventionalism.

Duhem, like many of Poincaré's self-professed successors [1], understands conventions as principles that are "incapable of being refuted by experiment" [Duhem 1906, 208]. The axioms of Euclidean geometry constitute one such set of conventions because they cannot be disconfirmed by such experiments as the measurement of stellar parallaxes. Poincaré maintains that should we discover negative parallaxes, we could either abandon Euclidean geometry or reject the assumption that light travels in straight lines. And since Euclidean geometry will always be preferable to its non-Euclidean alternatives, Poincaré concludes that it "has nothing to fear from fresh experiments" [Poincaré 1902, 73].

Duhem concedes that there are statements that are not straightforwardly verifiable or falsifiable, but he denies that they lack "experimental meaning". The principle of inertia is one such hypothesis and is explicitly identified by Poincaré as having the status of a convention. Duhem maintains that it is supposed to have this status because the motions of bodies cannot be determined without first identifying a frame of reference, and we will always be in a position to select a frame in which the law of inertia holds [Duhem 1906, 213]. He points out, though, that the laws of motion are not unique in this regard. In fact, Duhem maintains that *any* principle in the mathematical sciences can be insulated from experimental disconfirmation. He says, for example, that "no chemical analysis, no matter how refined, will ever be able to show the law of multiple proportions to be wrong" [Duhem 1906, 214], because we will always be free to change our initial assumptions about the relative masses of the constituents of the compounds involved in a chemical reaction. If the distinguishing feature of conventions is that they are insensitive to empirical facts, and yet *any* statement can be maintained in the face of experience, then the distinction between conventions and factual statements is arbitrary.

Furthermore, from the fact that these principles do not admit of decisive experimental tests, it does not follow that empirical evidence cannot compel us to revise them. The laws of motion and the law of multiple proportions constitute the foundations of Newtonian mechanics and modern chemistry, respectively. And though these theories have proven remarkably effective in predicting and explaining certain features of our experience, there may come a day when we are best served to abandon them—indeed, such a day has come

1. See [Neurath, Hahn, & Carnap 1929, 312], [Schlick 1915, 168–169], [Carnap 1995, 144–145].

and gone in the case of Newtonian mechanics. Foundational hypotheses, then, are only as good as the theories that they make possible, and should these theories come into conflict with experience, their presuppositions will have been refuted by experiment, albeit indirectly. Thus, no statement is immune to revision, and so the distinction between factual and conventional statements is non-existent.

Duhem's objection to Poincaré's conventionalism crucially relies on his characterization of conventions as principles that we choose to insulate from empirical disconfirmation. It will be argued, however, that this characterization is overly simplistic and misleading; to properly understand Poincaré's conventionalism, we must differentiate between the principles that *must* be invoked to apply our theoretical concepts to experience—we will call these *constitutive principles*—and the principles that we are *free* to invoke in applying our concepts to experience—we will call these *conventions.* The former are *implicitly* and necessarily presupposed in our measurement of theoretical quantities, even though we do not always realize that this is the case, while the latter we choose to *elevate* to the status of conventions. This explains why, in his philosophical writings on geometry, mechanics, and electrodynamics, Poincaré insists on the definitional status of certain principles while recognizing that the epistemological status of other principles will be determined by considerations of expedience. Thus, while Poincaré will readily admit that an *exhaustive* distinction between factual and conventional statements cannot be made on anything other than pragmatic grounds, he will deny that *any* statement can play the interpretive role of a constitutive principle, as Duhem's objection supposes. Finally, it will be argued that while Poincaré had thought that constitutive principles cannot be revised, his insights into their definitional character nevertheless provide some of the resources necessary to differentiate their revision from changes in our empirical beliefs.

1 Interpretations of Poincaré's conventionalism

Several readers have interpreted Poincaré's conventionalism in much the way that Duhem does.[2] Friedman, however, presents a reconstruction of Poincaré's conventionalism with the express purpose of differentiating it from the epistemological holism of Duhem and Quine, thereby avoiding their argument against the possibility of drawing a non-arbitrary fact-convention distinction. The key to understanding Poincaré's conventionalism, Friedman thinks,

2. See, for example, [Giedymin 1982], [Giedymin 1991], [Gillies 1993, Chapter 4], [Ben-Menahem 2001], [Ben-Menahem 2006, Chapter 2]. Only Giedymin provides a response to the argument from epistemological holism.

is to appreciate his view that the exact sciences sit in a hierarchical order of dependence: The theory of mathematical magnitude presupposes arithmetic, geometry presupposes the theory of mathematical magnitude, the laws of motion presuppose geometry, and the empirical laws of physics presuppose the laws of motion. The laws of motion cannot be tested empirically because they define the concepts of motion, force, and mass that we must take for granted in any such experimental test. The situation is somewhat more complicated in the case of geometry. For Poincaré, geometry is the study of the free displacements of rigid bodies (see § 2 below). These motions, he finds, constitute a six-dimensional Lie group; the Helmholtz-Lie theorem establishes that the existence of such a group implies that space must possess a constant curvature, but it does not specify that the curvature be positive (in the case of Riemannian geometry), negative (in the case of Bolyai-Lobachevsky geometry), or null (in the case of Euclidean geometry). Experiment cannot make this determination either, for we are not actually acquainted with rigid bodies, since all bodies are subject to physical forces. Friedman concludes:

> It is therefore completely impossible simply to read off, as it were, geometry from the behavior of actual bodies without first formulating theories about physical forces. [...] And it now follows that geometry cannot depend on the behavior of actual bodies. For, according to the above-described hierarchy of sciences, the determination of particular physical forces presupposes the laws of motion, and the laws of motion in turn presuppose geometry itself: one must first set up a geometry before one can establish a particular theory of physical forces. [Friedman 1999, 78]

The principles of geometry, like the laws of motion, are not subject to empirical tests because such a test will take place within a science—empirical physics—that necessarily presupposes the laws of motion, and thus, the principles of geometry. These principles are not statements that we choose to regard as conventions, but definitions that must be in place before the testing of empirical hypotheses can begin.

One of the appealing features of Friedman's reconstruction of Poincaré's conventionalism is that it accounts for the conventionality of the principles of geometry *and* the basic laws of physics. Nevertheless, it seems to me that Euclidean geometry and the laws of motion are not conventional in the same sense for Poincaré. Friedman's account of the conventional status of the laws of motion is quite in order. He says, with regard to Poincaré's argument:

> I understand him here to be arguing that the fundamental concepts of time, motion, mass, and force have no determinate empirical meaning *independently* of the laws of mechanics. Thus, for example, the laws of motion supply us with an implicit definition of the inertial frames of reference, without which no empirically applicable concept of time or motion is possible; the concepts

> of mass and force are only empirically applicable on the basis of the second and third Newtonian laws of motion; and so on. [Friedman 1999, 76]

The laws of motion function as 'definitions in disguise' insofar as they provide us with the conditions that *must* be met if force and mass are to be measurable, i.e., empirically applicable, quantities. Their definitional status is due to what Friedman elsewhere calls their *constitutive* role within our physical theory [Friedman 2002]. A principle p is a constitutive condition of a set of theoretical statements T, when one or more of the terms in T is *meaningless* in the absence of p. Without the laws of motion, the principles of mechanics, such as the law of universal gravitation, would fail to make empirical sense.

Poincaré does not present the same sort of argument for his geometrical conventionalism. The postulates of Euclidean geometry do not express the conditions that must be met if size and distance are to be measurable quantities; these are expressed by the principle of free mobility, which is compatible with all three geometries of constant curvature. Instead, the conventional status of the principles of geometry is due to our freedom to *choose* which objects remain practically rigid during spatial transport. This freedom is guaranteed not in virtue of the fact that the laws of motion must take for granted a set of geometrical principles, but by the fact that one and the same observable state of affairs will *always* be describable using either Euclidean geometry together with a set of physical laws or Lobachevskian geometry together with a very different set of laws. This is precisely the point of Poincaré's scenario of a closed spherical world in which the temperature at every point is proportional to $R^2 - r^2$, where R is the radius of the sphere and r is the distance of the point from the center of the sphere, and in which the size of all bodies also varies in accordance with this law [Poincaré 1902, 65–68].[3] From our Euclidean perspective, the inhabitants of this world and their measuring instruments shrink as they approach the boundary of the sphere. From their perspective, they live in an infinite Lobachevskian world in which bodies are displaced without being deformed. Since these amount to observationally equivalent descriptions of the same set of facts, we are free to use either notion of rigidity.

The key to understanding Poincaré's conventionalism, as I will argue below, is not to be found in the hierarchical ordering of the sciences, but in the hierarchical ordering of *principles* within the sciences.[4] Geometry must take for granted notions of shape and size, as they are defined by the principle of free mobility. Once we have adopted a geometry that is compatible with the condition of free mobility, we can then make particular empirical determinations of the sizes and shapes of the figures presented to us in experience. We are

3. He also supposes that in this world light travels through media whose indices of refraction are inversely proportional to $R^2 - r^2$.

4. Illuminating work on Poincaré's typology of hypotheses can be found in [Heinzmann 2008], [Walter 2008], and [Walter 2010].

free to adopt a non-Euclidean geometry for this purpose, but from Poincaré's perspective, considerations of simplicity will prevent us from actually doing so. In brief, certain empirical results must be understood against the background of pragmatically constrained *conventions,* which, in turn, must presuppose the *constitutive principles* that make it possible to apply our theoretical concepts to experience. As we shall see, this schema of principles is discernible not only in Poincaré's work on geometry, but also in his writings on mechanics and electrodynamics. Moreover, it will be argued that Duhem's failure to appreciate this subtle distinction undermines his argument from epistemological holism.

2 Conventions in geometry

According to Poincaré, the principle of free mobility is essential to our knowledge of space in three respects. First, it is indispensable to the *genesis* of our understanding of space; Poincaré maintains that at an early age we learn to distinguish spatial displacements from other kinds of alterations in our perceptual field by classifying the former as changes that can be undone by a suitable reorientation of our bodies. This capacity of ours, however, depends on the presupposition that bodies can be displaced without thereby undergoing a deformation. Second, the constructive proof procedures used in synthetic geometry necessarily take for granted the principle of free mobility. In synthetic geometry, two figures are equal when we can superpose one on the other, but

> This assumes that they can be displaced and also that, among all the changes which they may undergo, we can distinguish those which may be regarded as displacements without deformation. [Poincaré 1898, 32]

Without this presupposition we would lack well defined notions of the shape and size of geometrical constructions [Poincaré 1898, 33]. Finally, when we measure the shape and size of physical bodies, we presuppose that our measuring instruments are not deformed when transported from one place to another. Thus, the principle of free mobility makes possible the application of our geometrical concepts to physical objects.

The distinctive status of the principle of free mobility is not the result of our choosing to hold it true "come what may", but instead, is due to its *constitutive* function within the science of space. It is this constitutive function that Poincaré has in mind when he argues against Russell that the principle expresses a disguised definition of shape rather than a factual claim about something with which we are antecedently familiar. [5] Russell maintains that 'shape'

5. For an illuminating discussion of the debate between Russell and Poincaré on this topic, see [Coffa 1986, § 2].

is an indefinable term whose meaning must be immediately apprehended in intuition. The principle of free mobility, accordingly, is not a definition, but an *a priori* truth whose justification is secured by particular features of our intuition of bodies in space. Our intuitive notion of shape, however, relies on our ability to view a figure from different perspectives, and so, takes it for granted that our bodies can move freely through space. Moreover, Poincaré argues that these intuitions tell us nothing about how to actually *determine* the shape and size of geometrical figures. The principle of free mobility, on the other hand, plays a crucial role in making such determinations possible; the way we determine that two or more figures have the same shape and size is by bringing them into congruence. When doing so, we assume that their spatial dimensions do not change. Therefore, shape cannot be defined independently of the principle of free mobility, and so this principle has the status of a *definition* rather than an *a priori* truth [Poincaré 1902, 44–45].

The principle of free mobility, however, does not tell us *which* bodies remain rigid during spatial transport. This is because, as we have seen, Euclidean and Lobachevskian geometries constitute observationally equivalent descriptions of the same set of facts. We are free to *choose* either one of them as determining what will count as a rigid body and a straight line.[6] And because Euclidean geometry is the simplest candidate, we stipulate that rigid bodies and straight lines will behave in our world the way that Euclidean geometry says they do. This conventional *choice,* though, ought to be distinguished from the *constitutive principle* that makes this choice possible, namely, the principle of free mobility. The principle of free mobility is not stipulated at the outset, but is implicitly *presupposed* in our measurements of geometrical figures.

3 Conventions in mechanics

Just as the principle of free mobility is constitutive of our notion of space, Poincaré maintains that the laws of motion are constitutive of our notions of force and mass. If we are to empirically test the second law of motion, says Poincaré, "we have to measure the three magnitudes mentioned in the enunciation: acceleration, force, and mass" [Poincaré 1902, 97]. More specifically, we must be able to measure force, mass, and acceleration without appealing to Newton's second law. Poincaré admits that we can detect and measure acceleration independently of the second law of motion, provided we have a reliable means of measuring time. Forces, on the other hand, are not the kinds of things that can be directly observed. Nevertheless, we may indeed be tempted, as was Russell with regard to the principle of free mobility, to claim that the second law of motion captures some independently discernible fact

6. Alternatively, we may designate certain physical entities as rigid bodies and straight lines and determine the geometry empirically.

about our intuitive understanding of force. However, Poincaré insists that any such primitive understanding of force as, for example, the muscular exertion needed to displace an object, is an "insufficient basis for mechanics", because it does not tell us how force is to be *measured* [Poincaré 1902, 106]. The second law of motion, on the other hand, tells us exactly how to detect and measure forces: we do so by measuring their observable effects on physical bodies, namely, their tendency to alter a body's state of motion. Since we cannot define force independently of the second law of motion,

> We are therefore reduced to Kirchoff's definition: force is the product of the mass and the acceleration. This law of Newton in its turn ceases to be regarded as an experimental law, it is now only a definition. [Poincaré 1902, 100]

Using this definition, as it stands, we can measure two forces when applied at different times to one and the same body, but it does not permit us to measure the action of two forces when applied to distinct bodies until we have a way of determining the masses of those bodies. We must do this by assigning to each body within the system under consideration a mass such that the motion of the system's center of gravity is uniform and rectilinear. Poincaré insists that:

> This will always be possible if Newton's third law holds good, and it will be in general possible only in one way. [Poincaré 1902, 103]

Once again, we see one of Newton's laws functioning as a definition, this time of 'mass', rather than an empirical or *a priori* truth. Things are not so simple, however:

> But no system exists which is abstracted from all external action; every part of the universe is subject, more or less, to the action of the other parts. *The law of the motion of the centre of gravity is only rigorously true when applied to the whole universe.* [Poincaré 1902]

If we are to rigorously define mass, then we must be able to locate the universe's center of gravity. Since this task is impossible, we must settle for the definition:

> *Masses are co-efficients which it is found convenient to introduce into calculations.* [Poincaré 1902]

Poincaré adds that:

> We could reconstruct our mechanics by giving to our masses different values [than the values we currently give them] ... But the equations of this mechanics would not be so simple. [Poincaré 1902, 103–104]

The situation in mechanics closely resembles Poincaré's assessment of the situation in geometry. The laws of motion, like the principle of free mobility, function as *constitutive principles* which implicitly define the terms 'force' and 'mass', thus making it possible to solve Newton's "basic problem of philosophy [which] seems to be to discover the forces of nature from the phenomena of motions and then to demonstrate the other phenomena from these forces" [Newton 1726, 382]. However, just as the principle of free mobility is compatible with any geometry of constant curvature, and thus fails to single out any one of them as constituting the correct description of space, the "general principles of mechanics" (the constitutive principles) do not uniquely fix the "equations of mechanics" (the empirical laws relating to particular forces and masses), and so there is a certain measure of choice involved in our dynamical description of the forces and masses at work in the universe [Poincaré 1902, 104]. The assignment of masses to a dynamically interacting system of bodies will be constrained by empirical evidence regarding the accelerations of the bodies, but these constraints are not strong enough to single out a unique assignment of masses as being correct.

4 Conventions and the measurement of time

Like the case of force, we have an intuitive notion of the passage of time; there is a clear intuitive sense in which our experiences occur in a temporal sequence, and that two experiences can happen simultaneously. It is not at all clear, though, that our experience of the passage of time coincides with the experiences of others, nor that we can use these experiences to objectively *measure* time. In fact, Poincaré maintains that:

> *We have not a direct intuition of the equality of two intervals of time.* [Poincaré 1905, 27]

Rather than appeal to their intuition, physicists use pendulums to measure duration by supposing that each beat takes the same amount of time. There are, however, various disturbing influences on the pendulum (temperature, air-resistance, barometric pressure, etc.) which compel us to correct the time it measures. We do so by comparing it with yet another standard: the sidereal day. Physicists suppose, though, that the tides act as a perturbing influence on the rotation of the earth, which explains the apparent acceleration of the moon, and as such, even this standard must be corrected. What these facts about our measurement of time intervals tell us is not that there is an absolute time in which all physical events occur, but that our means of measuring time presuppose an ideal that we can approximate only roughly. This ideal is determined by the principle that is presupposed in every measurement of temporal intervals:

> When we use the pendulum to measure time, what postulate do we implicitly admit? *It is that the duration of two identical phenomena is the same*; or, if you prefer, that the same causes take the same time to produce the same effects. [Poincaré 1905, 28]

This is the principle independently of which we cannot make sense of our notion of time. The problem with actually *applying* this postulate is that physical phenomena are not causally isolated; what seem like two instances of the same phenomenon—say, two swings of a pendulum—actually involve countless perturbing influences. Therefore, we must invoke physical principles to correct our standards of measurement. The law of the conservation of momentum suggests that the rotation of the earth is a physical process that can be used to reliably measure time intervals. When measuring temporal intervals using sidereal days, however, we must take into account the heat produced by the friction of the tides to explain the secular acceleration of the moon. In this case, the law of the conservation of energy and the law of universal gravitation are being used to correct our standard of measurement. There is nothing preventing us from treating our standard as precisely true and altering our physical laws, except that this course of action would make the science of mechanics overly cumbersome:

> So that the definition implicitly adopted by the astronomers may be summed up thus: time should be so defined that the equations of mechanics may be as simple as possible. In other words, there is not one way of measuring time more true than another; that which is generally adopted is only more *convenient.* [Poincaré 1905, 30] [7]

There are two kinds of conventions that make possible the measurement of temporal intervals: First, the *definition* that the same causes take the same time to produce the same effects, and second, our *decision* to apply this definition such that our physics is made as simple as possible. The latter sort of convention is necessary because there are many observationally equivalent ways of measuring time; but the very notion of a measure of time makes sense only on the supposition that there exists a phenomenon that repeats itself uniformly.

Determining the temporal sequence of some set of physical events requires that we are able to determine which events occur simultaneously. Two psychological events occur simultaneously, says Poincaré, when "... analysis can not separate [them] without mutilating them" [Poincaré 1905, 31]. We can and do use this as a criterion to determine the order of *local* physical events, but of course it is not a definition of simultaneity that is applicable to distant physical events. In order to arrive at a definition of simultaneity that is applicable to all physical events, Poincaré proposes "to understand the definition implicitly supposed by the savants, let us watch them at work and look for the rules by which they investigate simultaneity" [Poincaré 1905, 34]. So, for

7. Here it should be recalled that Poincaré uses the terms "equations of mechanics" to designate empirical laws, and thus, they should not be confused with what he calls the "general principles of mechanics", which are the constitutive principles (the laws of motion) that make these laws intelligible.

example, what does an astronomer mean when he tells us that the light that we see now was emitted from a star some time ago? Poincaré answers,

> He has begun by *supposing* that light has a constant velocity, and in particular that its velocity is the same in all directions. That is a postulate without which no measurement of this velocity could be attempted. [Poincaré 1905, 34]

In order to put celestial events into a temporal sequence, we must know how far the light signals emitted from such events have to travel to reach us and we must *suppose* that these signals have a constant velocity in all directions. If we have the means to determine the distance between the earth and celestial bodies, and we assume that the light signals emitted from every body travel at the same speed, then we can determine the sequence of celestial events. Furthermore, by combining this rule with the principles of mechanics, we can determine how fast the light signals are traveling, and thus, how long ago celestial events that we experience now took place. We could, for example, determine the velocity of light by determining the lag between the observed eclipses of the satellites of Jupiter and the Newtonian prediction of when these events would take place. In doing so, of course, we would have to assume the law of universal gravitation to be precisely true, but we are certainly not forced to do so:

> Could not the observed facts be just as well explained if we attributed to the velocity of light a little different value from that adopted, and supposed Newton's law only approximate? Only this would lead to replacing Newton's law by another more complicated [law]. So for the velocity of light a value is adopted, such that the astronomic laws compatible with this value may be as simple as possible. [Poincaré 1905, 35] [8]

Once again, there are two different kinds of conventions at work in determining when a distant event took place: first, the *definition* that light has a constant velocity in all directions, and second, the *decision* to treat the law of universal gravitation as being precisely true. The latter is chosen on the basis of its pragmatic advantages over its observationally equivalent alternatives, the former is a constitutive principle that makes the temporal ordering of (distant) physical events possible.

5 Constitutive principles and epistemological holism

Duhem recognizes the need for definitions which confer "symbolic meanings" on abstract theoretical terms by linking them with observable phenom-

8. Since Roemer attempted this calculation before the publication of Newton's *Principia,* one could make a similar remark about the status of Kepler's laws.

ena [Duhem 1906, 194]. In his view, however, the principles that serve this function are no less empirical than other hypotheses within the mathematical sciences. For example, he explains that the expression 'free falling heavy body' is given a symbolic meaning by the law of falling bodies. Nevertheless, in the event that we are faced with a situation in which a seemingly unsupported body does not fall with a constant acceleration, we may reject the law of falling bodies and change our physics accordingly, or alternatively, we may postulate the existence of unseen obstacles that hindered the fall of the object in question, such as air resistance. The latter option will almost always be preferable since "taking the first alternative we should be obliged to destroy from top to bottom a very vast theoretical system which represents in a most satisfactory manner a very extensive and complex set of experimental laws" [Duhem 1906, 211]. We are not, however, logically prevented from taking the former course of action, and in fact there will be exceptional circumstances in which the revision of such definitions results in the genesis of a revolutionary new theory. Thus, Duhem thinks that because of the underdetermination of theory by observation, there is no principled distinction to be drawn between definitions and empirical laws; any principle within the corpus of our scientific knowledge is liable to be elevated to the status of a definition, while at the same time, any definition can be revised, should we find it convenient to do so.

Poincaré recognizes that there are principles that can be treated either as empirical laws or as definitions. He says, for example, that we may adopt the attitude that the law of universal gravitation is an empirical law, or we can elevate it to the status of a definition, and he adds:

> The choice between the two attitudes is free, and is made from considerations of convenience, though these considerations are most often so strong that there remains practically little of this freedom. [Poincaré 1905, 124]

As Duhem points out, we may adopt either one of these attitudes with respect to *any* principle within the mathematical sciences.

This insight, however, does not succeed in establishing that Poincaré's fact-convention distinction is unavoidably arbitrary. Constitutive principles are unrevisable not because we have chosen to treat them as such, but in virtue of their interpretive function of *expressing the minimal theoretical structure required to make empirical sense of the fundamental abstract terms of our theories.* Furthermore, Duhem fails to realize that constitutive principles cannot be underdetermined by empirical evidence because evidence in the mathematical sciences ultimately rests on the sorts of measurement results that are made possible by such principles as the laws of motion and the principle of free mobility. We are not free, as we are in the case of the law of falling bodies or the universal law of gravitation, to regard these principles as definitions *or* empirical laws; instead, their constitutive status is determined by the fact that they make certain kinds of empirical tests possible. Consequently, the holist's observation that "[a]ny statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system" [Quine 1961, 43]—an observation that Poincaré may well have endorsed—does not motivate a move from Poincaré's conventionalism to "a more thorough pragmatism" [Quine 1961, 46], since it clearly does not follow from this observation that any statement can be considered a constitutive principle.

6 The revision of constitutive principles

Poincaré's insights into the constitutive character of certain physical principles clearly motivate his view of the stratified structure of scientific knowledge. One of the aspects of this view, as we have just seen, is that the constitutive component of scientific knowledge is immune to revision because it gives empirical content to our theoretical concepts. This aspect of Poincaré's view, however, is challenged by the holist's claim that "no statement is immune to revision", and definitively undermined by the course of physics in the twentieth century; indeed, we know that the principles Poincaré identifies as being constitutive are revisable because nearly all of them have been displaced by the theories of relativity.[9] Nevertheless, the pertinence of these facts to Poincaré's conventionalism ought not to be overstated. Rather than being mistaken about the distinction between empirical and constitutive principles, we might suspect that Poincaré was mistaken in thinking that the interpretive function of the latter makes them immune to revision. Making this case, though, puts us in the position of having to rise to the naturalist's challenge "to differentiate revisions in the purportedly *a priori* claims from ordinary scientific progress" [Maddy 2000, 114].

This challenge has been taken up recently by Friedman's theory of the relativized *a priori*. Like Poincaré, he claims that abstract scientific theories must be formulated and evaluated within a constitutive framework that defines what is physically possible and what will count as evidence for and against such theories [Friedman 2002, Part II, §§ 1–2]. This being the case, Friedman says:

> Our problem, then, is to explain how a revolutionary transition from one scientific paradigm or constitutive framework to another can be communicatively rational, despite the fact that we are in this case faced with two essentially different and even incommensurable "logical spaces". [Friedman 2002, 95]

His answer is that it is at the meta-scientific level of *philosophical* reflection that we first encounter reasons to take new constitutive frameworks se-

9. The only one that remains is his definition of simultaneity.

riously. So, for example, while the special theory of relativity fails to engage classical mechanics on its own terms, it does constitute a contribution to the ongoing philosophical debates concerning absolute versus relative motion and the status of theoretical principles. Friedman maintains that Einstein's main philosophical insight was to raise the relativity and light principles to the status of "postulates", and it is in precisely this move that he sees the influence of Poincaré:

> Einstein proceeded here, in perfect conformity with Poincaré's underlying philosophy in *Science and Hypothesis,* by "elevating" an already established empirical fact into the radically new status of what Poincaré calls a "definition in disguise"—namely, what we call a coordinating principle. [Friedman 2002, 111]

By elevating the relativity principle and the light principle to the status of postulates, Einstein produces an entirely novel theory of space and time, or rather space-time, in which the laws of electrodynamics, rather than those of mechanics, play the role of a constitutive framework. And while the classical physicist and the proponent of special relativity are essentially speaking different languages—they will measure length and time differently, and thus the terms 'length' and 'time' have entirely distinct meanings for these two physicists—the classical physicist cannot ignore Einstein's theory as nonsense because it engages contemporary *philosophical* concerns about space and time, and the status of fundamental theoretical principles.

Friedman's account of the rationality of revolutionary theory change and his account of Poincaré's role in the genesis and acceptance of special relativity conflicts with the view of Poincaré's conventionalism outlined above. It has been argued that constitutive principles—the sorts of principles that were revised in the move from classical mechanics to special relativity—are not *elevated* to the status of 'definitions in disguise' but *implicitly presupposed* in our measurement of theoretical magnitudes. It must be admitted, however, that when faced with Einstein's kinematical interpretation of the Lorentz symmetry, Poincaré himself says:

> Today some physicists want to adopt a new convention. It is not that they are constrained to do so; they consider this new convention more convenient; that is all. [Poincaré 1913, 24]

These remarks seem to constitute an admission on Poincaré's behalf that the fundamental principles of Newtonian physics do not in fact have the constitutive status that he had taken them to have, since he now thinks that they have been adopted, and may be revised, on pragmatic grounds. Indeed, it is now difficult to see in what respects Poincaré differs from Duhem, who acknowledges that we grant to certain principles the status of definitions with the understanding that in the future it may make "good sense" to give them up. If this conclusion is to be avoided, and the present view of Poincaré's conventionalism is to be sustained, there must be an alternative account of the revision of constitutive principles that is sensitive to their intrinsically interpretive character. The remainder of this paper will be dedicated to showing that such an account of Einstein's analysis of simultaneity can be defended, and in fact, is quite plausible.[10]

Despite his talk of raising the light principle to the status of a postulate, what Einstein actually shows is that the light principle *cannot* be empirically tested, and therefore, *cannot* be considered an empirical law. In order to measure the speed of light, one must *already* be able to construct an inertial frame of reference, i. e. a frame in which departures from inertial motion can be accounted for by the presence of forces. Although, as Einstein points out:

> If we wish to describe the *motion* of a material point, we give the values of its co-ordinates as functions of the time. Now we must bear carefully in mind that a mathematical description of this kind has no physical meaning unless we are quite clear as to what we understand by "time". [Einstein 1905, 39]

If the light principle plays an *essential* role in the construction of inertial frames, then any experiment designed to test this principle will necessarily be circular. Consequently, Einstein's view that the light postulate functions as a constitutive principle rests on its indispensability to our spatio-temporal framework. Einstein's argument that this is indeed the case rests on his analysis of simultaneity.

Einstein maintains that:

> We have to take into account that all our judgments in which time plays a part are always judgments of *simultaneous events.* [Einstein 1905, 39]

Consequently, if we are going to become clear on what we understand by "time", we must also be clear on what we understand by "simultaneous events". Here Einstein is not looking to stipulate a new definition of simultaneity, but like Poincaré, to "understand the definition [of simultaneity] supposed by the savants" [Poincaré 1905, 34]. When making judgements about the sequence of local events, savants and laymen alike use the same criterion of simultaneity: two (local) events are simultaneous when they are seen at the same time, i. e., when light signals emitted from those events are perceived at the same time. This is not a suitable criterion for determining sequences of distant events, but Poincaré and Einstein recognize that here too light signals are used:

> We have not defined a common "time" for [observers] A and B, for the latter cannot be defined at all unless we establish *by definition* that the "time" required by light to travel from A to B equals the "time" it requires to travel from B to A. [Einstein 1905, 40]

10. The remainder of this section is heavily indebted to [DiSalle 2006, Chapter 4.2].

Like Poincaré, however, we may think of this as being a definition of "local time" and that the true sequence of events can be determined only by the use of infinitely fast signals (gravitational signals, perhaps). The true order of events can be reconstructed on the basis of the local procedure, but this will involve appealing to the velocity addition law to determine the time it takes for light to travel from the events to the observer. This process is undermined if, as it would seem, light does not obey the classical velocity addition law. In this case, Einstein's definition can no longer be regarded as a provisional substitute for an invariant criterion; it is, in fact, itself an invariant criterion since it has the unique advantage of being "independent of the standpoint of the observer" [Einstein 1905, 39]. As a result, we see that the light principle is indeed essential to our spatio-temporal framework, and for this reason, cannot be considered an empirical law.

Poincaré had thought of constitutive principles as being unrevisable, and when, in the face of special relativity, it became obvious that this is not the case, he offers an account of the choice between alternative spatio-temporal frameworks that effectively collapses the important distinction between constitutive principles and conventions. What Einstein's treatment of simultaneity reveals, however, is that the move from one constitutive framework to another is not happily seen as a choice between two incommensurable languages or paradigms. Instead, constitutive principles are revised on the basis of *conceptual analyses* that uncover the assumptions that make the empirical application of our theoretical concepts possible. Einstein's argument begins with a criterion of simultaneity that *both* he and his opponents (Poincaré and Lorentz) accept, at least provisionally. The crucial move is then made when he shows that, using this criterion, we cannot reconstruct an objective temporal ordering of events, and yet as far as we know, this is the *only* available criterion that is independent of the position and velocity of the observer, and consistent with well established physical laws.

Consequently, what Einstein has shown is not that the light principle can be elevated from an empirical law to the status of convention, thereby giving us a radically new definition of simultaneity, but that it was a mistake to regard the light principle as being an empirical law in the first place, for it plays an indispensable role in the construction of a spatio-temporal framework in which empirical laws make sense. Poincaré was sufficiently radical as a philosopher to see how such an analysis is capable of establishing the constitutive status of certain principles, as in the case of the principle of free mobility, but insufficiently radical as a mathematical physicist to see how such an analysis is capable of revealing *new* constitutive principles.

Conclusion

Poincaré's contributions to axiomatics, group theory, and geometry suggested to him that there is an important distinction to be made between the structure and content of mathematical theories. It was natural, therefore, for him to question how these two components relate to one another; in particular, Poincaré was concerned with how theories about such abstract entities as space, time, and force could have anything to do with experience. He had no doubt, of course, that we experience bodies in space and time, and under the influence of forces, but these experiences by themselves have little bearing on our scientific theories because they do not tell us how the quantities in question are to be measured. Our observations become theoretically informative, he argues, only when we *presuppose* certain constitutive principles which link our abstract concepts to experience. The conventional status of these principles is due not to their being stipulated, arbitrary, or immune to revision, but to their role as definitions rather than empirical claims containing antecedently defined terms. These definitions make the measurement of the quantities designated by our basic theoretical terms possible, though they do not specify precisely how they are to be measured; this is accomplished by a further sort of convention which we *choose* to hold true in the face of empirical evidence. The epistemological naturalist's observations that any statement can be insulated from empirical disconfirmation, and yet no statement is beyond getting revised when it makes good sense to do so, thus do not impugn Poincaré's conventionalism because her characterization of a convention as a statement that is insensitive to empirical evidence fails to capture the interpretive role that these principles play in turning observations into evidence.[11]

Bibliography

Ben-Menahem, Yemima

2001 Convention: Poincaré and some of his critics, *British Journal for the Philosophy of Science*, 52, 471–513.

2006 *Conventionalism*, Cambridge: Cambridge University Press.

Carnap, Rudolf

1995 *An Introduction to the Philosophy of Science*, New York: Dover.

Coffa, J., Alberto

1986 From geometry to tolerance: Sources of conventionalism in nineteenth-century geometry, in *From Quarks to Quasars, University*

11. I owe a significant debt to Robert DiSalle for his generous and insightful guidance, advice, and comments. I would also like to thank Aaron Barth, Sona Ghosh, Kathleen Okruhlik, and Christopher Smeenk for their comments on earlier drafts.

of Pittsburgh Studies in the Philosophy of Science, edited by Colodny, R., Pittsburgh: University of Pittsburgh Press, 3–70.

Devitt, Michael

1998 Naturalism and the a priori, *Philosophical Studies*, 92, 45–65.

2005 There is no a priori, in *Contemporary Debates in Epistemology*, Oxford: Blackwell, 105–114.

DiSalle, Robert

2006 *Understanding Space-time*, Cambridge: Cambridge University Press.

Duhem, Pierre

1906 *La Théorie physique; son objet, sa structure*, Paris: Chevalier et Rivière, 2nd ed., translated by P. Wiener as: *The Aim and Structure of Physical Theory,* Princeton: Princeton University Press, 1954 .

Einstein, Albert

1905 On the electrodynamics of moving bodies, in *The Principle of Relativity*, edited by A. Einstein, H. Minkowski, H.A. Lorentz & Weyl, H., New York: Dover, 37–51, translated by W. Perrett and G. B. Jeffrey, New York: Dover, 1952.

Friedman, Michael

1999 *Reconsidering Logical Positivism*, Cambridge: Cambridge University Press, chap. Poincaré's Conventionalism and the Logical Positivists, 71–86.

2002 *The Dynamics of Reason*, Chicago: University of Chicago Press.

Giedymin, Jerzy

1982 *Science and Convention: Essays on Henri Poincaré's Philosophy of Science and the Conventionalist Tradition*, Oxford, New York: Pergamon Press.

1991 Geometrical and Physical Conventionalism of Henri Poincaré in Epistemological Formulation, *Studies in History and Philosophy of Science*, 22(1), 1–22.

Gillies, Donald

1993 *Philosophy of Science in the Twentieth Century*, Oxford: Blackwell.

Heinzmann, Gerhard

2008 Hypotheses and conventions in Poincaré, in *The Hypothetical in the Natural Sciences*, edited by Heidelberger, M. & Schiemann, G., Berlin: De Gruyter, 169–192.

MADDY, PENELOPE

2000 Naturalism and the a priori, in *New Essays on the A Priori*, edited by BOGHOSSIAN, P. & PEACOCKE, C., Oxford: Clarendon Press, 92–116.

NEURATH, OTTO, HAHN, HANS & CARNAP, RUDOLF

1929 *Wissenschaftlische Weltaufassung: der Wiener Kreis, Veröffentlichungen des Vereines Ernst Mach*, Wien: Arthur Wolf Verlag, translated by O. Neurath as *The Scientific Conception of the World: The Vienna Circle,* in R. S. Cohen and M. Neurath (eds.) *Empiricism and Sociology,* Dordrecht: Reidel, 1973, 299–318.

NEWTON, ISAAC

1726 *The Principia: Mathematical Principles of Natural Philosophy*, Los Angeles: University of California Press, translated by I. B. Cohen and A. Whitman, 1999.

POINCARÉ HENRI

1898 On the foundations of geometry, *Monist*, IX, 1–43.

1902 *La Science et l'Hypothèse*, Paris: Flammarion, translated by W. J. Greenstreet as *Science and Hypothesis,* New York: Dover, 1952.

1905 *La Valeur de la science*, Paris: Flammarion, translated by G. B. Halsted as *The Value of Science,* New York: Dover, 1958.

1913 *Dernières pensées*, Paris: Flammarion, translated by J. Bolduc as *Mathematics and Science: Last Essays,* New York: Dover, 1963.

QUINE, WILLARD VAN ORMAN

1961 *From a Logical Point of View*, Cambridge: Harvard University Press, chap. Two Dogmas of Empiricism, 20–46.

SCHLICK, MORITZ

1915 Die philosophische Bedeutung des Relativitätsprinzips, *Zeitschrift für Philosophie und philosophische Kritik*, 159, 129–175, translated by P. Heath as "The Philosophical Significance of the Principle of Relativity", in H. L. Mulder and B. F. B Van de Velde-Schlick (eds.), *Moritz Schlick: Philosophical Papers,* vol. 1, Dordrecht: D. Reidel Publishing Company, 1979: 153–189.

WALTER, SCOTT

2008 Hypothesis and convention in Poincaré's defense of Galilei spacetime, in *The Significance of the Hypothetical in the Natural Sciences*, edited by HEIDELBERGER, M. & SCHIEMANN, G., Berlin: De Gruyter, 193–219.

2010 L'hypothèse naturelle, ou quatre jours dans la vie de Gerhard Heinzmann, in *Construction: Festschrift for Gerhard Heinzmann*, edited by P.E. Bour, M. Rebuschi & Rollet, L., London: College Publications, 129–135.